the geographical latitude. If, on the other hand, the trails of the two stars do not touch, their shortest distance may be measured. Now the distance of the trails of any other two stars, which appear during the same exposure, will, when their declinations are taken from the almanacks, enable us to calculate what angle approximately corresponds to a certain length, the approximation being close for small lengths. Thus we are enabled to calculate what the declination of the second star would have been, if its trail had touched the trail of the first star, and thus the latitude may be found.

Generally there will be the trails of several stars in both exposures available, so that one plate will allow of several determinations, and thereby of an elimination of errors. The results of seven plates, taken on different nights in my garden in Hanover, Germany, will give an idea of the accuracy.

52° 22′·50
52° 22′·56
52° 22′·47
52° 22′·78
52° 22′·90
52° 22′·93
52° 22′·90

Mean: 52° 22′·72 (mean error of a single plate = 0′·2).

According to the *Preussische Landesaufnahme* the latitude should be—

52° 22′·84

The same plate may also serve to determine the deviation of a watch from local mean time, if the time of the watch is noted each time the cap is withdrawn and replaced. Imagine, first, that a star was photographed whose declination coincided with the geographical latitude, and imagine that the box could be turned infinitely quickly without any oscillations being started. If we then found that the two trails of the star corresponding to the two exposures touched each other at one end, the end corresponding to the moment the box was turned, we might conclude that the star at this moment was in the zenith. For there alone the image of the star would remain unaffected by the turning of the plate. Now, we can calculate from the almanack the local mean time of the moment when this particular star culminates, and this would give us the deviation of our watch from local mean time. Imagine now the box had not been turned at the moment the star culminated, but, say, ten minutes later. Then the two trails would not have one end in common. The ends corresponding to the moment when the box was turned, would be on opposite sides of the zenith at the same distance from it. Now we should be able by the length of the trails to determine what interval of time corresponds to a given length, and so the plate would show us that the culmination of the star took place ten minutes before the box was turned. If the box is not turned very quickly, the interval after replacing and before removing the cap must be taken into account. But it is evident that we can calculate where the ends of the trails would have been if the box had been turned infinitely quickly. If there is no star that passes exactly through the zenith, we must take one that passes near the zenith. This will also do. By the ends of the trails we find which point of the plate corresponds to the zenith, and at the same time we can, by the trails and their ends, find out the line representing the meridian. This gives us the necessary data for finding the deviation of our watch from local mean time. For a star near to the zenith a small error in the direction of the meridian will make very little difference. I have spoken of the ends of the trails as a means of determining local mean time only for the sake of simplicity. It is far preferable to proceed differently, and to interrupt the first and the second exposure a number of times, thus causing as many interruptions of each trail. Five seconds or less will do very well for the length of an interruption. These interruptions are preferable to the ends, because they are symmetrical, and their middle can with more accuracy be brought under the cross-hairs of a micrometer. My lens having a focus of about 24 cm. made one minute about equal to 0·6 mm. on the trails of stars that culminate near the zenith. With a micrometer it is not difficult to measure exact within 0·01 mm. which is equivalent to one second of time.

If by the help of a chronometer Greenwich mean time were known at the place where the photograph was taken, the longitude would thus also be determined. But a chronometer can be dispensed with if another photograph is taken of the moon and the stars. For this purpose the camera is taken out of its box, directed to the moon, and fixed as firmly as possible. A number of instantaneous photographs of the lunar disc are then taken, all on the same plate, at intervals of not less than two minutes, to prevent the images from overlapping. The camera must be touched as little and as gently as possible, so that it may remain in the same position. After a number of exposures, say six or eight, the camera is shut until the moon has quite gone out of the field. The cap is then again removed, and the stars draw their trails over the plate, interrupted here and there, the lens being now and then covered for five seconds. If the local mean time of the instantaneous exposures of the lunar disc and of all the interruptions is known, the Greenwich mean time may be found from the plate. One may apply three different methods. Either one can determine the right ascension of the moon or the declination, or one can measure lunar distances. I have tried all three, and prefer the second, provided the moon is not near the maximum or minimum of her declination. For the slower rate of change of declination is made up by the greater accuracy with which it can be measured, on account of the trails being lines of constant declination. Each image of the lunar disc allows a separate determination, so that the final value is the mean of a number of determinations. (For the details of measuring, see my article in the *Zeitschrift für Vermessungswesen*, August 1893.)

The results of one plate for the difference of Greenwich mean time and local mean time were by the three methods—

39·1 minutes
39·1 ,,
38·6 ,,

Mean 38·93 ,,

while the true value is 38·943. This very close coincidence must be considered accidental. But I think one can well rely on the determination from one plate not differing more than 0·2 minute from the real value. When exposing for a longer time, the dew will sometimes condense on the lens, and render it opaque. To prevent this, I place a screen before the lens with a hole in it a little larger than the lens. The screen keeps the lens from cooling below the temperature of the surrounding air, and thus removes the cause for the condensation of dew. When determining the latitude the screen was arranged as a lid of the outer box, and served at the same time to protect the camera from gusts of wind.

C. RUNGE.

Technische Hochschule, Hannover.

Sodium and Uranium Peroxides, &c.

IN your notes of May 17 you give an account of work done by Prof. Poleck on the action of sodium dioxide on the salts of various metals, from the *Berichte* of May 8.

Some of this work has been already recorded by other observers who used either sodium dioxide or hydrogen dioxide in alkaline solutions.

In the *Journal* of the Chemical Society, 1877, pp. 1–24, and pp. 125–143, I gave papers on the reactions of hydrogen dioxide, and described its action on uranium salts both in acid and in alkaline solutions. I do not wish, however, to say anything on a mere question of priority, but simply to point out inaccuracies in the formulæ given in NATURE from the *Berichte*.

Prof. Poleck gives for sodium peruranate the formula $Na_4U_2O_8 . 8H_2O$. This either involves the use of the old atomic weight of uranium, half of that now adopted, or it means that his salt is exactly like that prepared by me in every respect, excepting that it contains twice the proportion of uranium. Assuming the atomic weight at approximately 240, the correct formula of sodium peruranate is $Na_4UO_8 . 8H_2O$ (*Journal* of the Chemical Society, 1877, p. 139).

I also showed that hydrated sodium dioxide is readily obtained on adding alcohol to mixed sodium hydrate and hydrogen dioxide solutions (p. 125), so that the materials used by Prof. Poleck and myself are practically the same.

The uranium compounds are interesting as examples of a special highly oxidised type (see Mendeléeff's "Principles of Chemistry," vol. ii. p. 244), also as throwing light on the nature of certain analogous but unstable bodies, such as perchromic acid. (*Journal* of the Chemical Society, 1877, pp. 7–8). This was

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further confirmed by Dr. C. Häussermann last year, who has isolated a definite crystallised sodium perchromate, as described in the *Journal für Praktische Chemie*, quoted in NATURE in the notes given July 27, 1893, p. 300. THOMAS FAIRLEY.

Cataloguing Scientific Papers.

THE recent circular issued by the Royal Society anent the indexing of scientific literature affords me a pretext for suggesting in your columns a reform which I have long thought to be urgently required. It is that henceforward all scientific publications should be issued in only one volume per annum—in parts, if necessary, but consecutively paged and with only one index—and that this volume should be primarily referred to by the year of its publication, *not* by its number since the first issue of the publication. Two advantages would accrue from this system. In the first place, the date of all quoted work would be fixed; in the second place, the finding of the abstracts of papers published elsewhere, printed in the journals of scientific societies, would be rendered more easy. A little reflection will show that these benefits are not trivial. For example, suppose an author refers to a paper by Smith published in NATURE, vol. xi. I have not (may I be pardoned for saying so!) the slightest idea when NATURE was first issued, nor do I remember whether one or two volumes of this periodical appear per annum. I am therefore totally in the dark as to whether Smith's work is one year old or twenty years old, and consequently I am ignorant whether he is likely to have used the most modern appliances in his research, and whether he is likely to have been contradicted by subsequent observers. Again, I am referred by an author to a paper by Schmidt, in the *Berichte* of the German Chemical Society, vol. xx. Not possessing this journal, I hope to be able to find an abstract of the paper in question in the *Journal* of the Chemical Society, to which I subscribe; but as I have no notion in what year vol. xx. of this *Berichte* was published, I have to search through numerous indexes in order to find the abstract. A search for previously published work is already sufficiently difficult to cause many to shrink from the task; ten years hence it may be expected to be the most laborious and thankless work which the investigator has to perform. A. G. BLOXAM.

May 19.

Clavatella Prolifera.

THIS hydrozoan may be added to the list of the Jersey marine fauna. It occurs in rock pools on the higher littoral between the Point des Pas and Gorey, and probably at other places round the coast. I often found three or four colonies in one small pool; but the number of polypites in a colony was very small—generally two or three, rarely four, and only in one case five. The stolon runs along in the chinks of the Melobesia that grows over so many of the pools, hence it is not an easy matter to obtain specimens there. The walking-buds, however, were fairly plentiful.

May I ask if any correspondent of NATURE has ever seen the walking-bud of *Eleutheria*, in which both extremities of the bifurcated arms are said to consist of a ball of thread-cells?

May 22. HENRY SCHERREN.

THE DESTRUCTIVE EFFECTS OF SMALL PROJECTILES.[1]

THE effects of small projectiles when driven at high velocity through the tissues of the brain have always excited the deepest interest, for very obvious reasons.

This interest must always be two-sided, namely: (1) Physical; (2) Pathological; and it is upon these two points of view that I propose to speak to you this evening.

Conceive a cylindrical bullet with a conical head flying through the air some ten or fifteen times faster than an express train.

We have now to study what it is doing in its aerial flight, and what will happen when that terminates by the projectile striking both hard and soft substances.

This embodies matter for the purely physical side of the work.

But imagine, further, that the hard and soft substances just mentioned are the skull and brain respectively, what will happen then?

This is the pathological part of the question, and it is one of the greatest moment; for whereas it is true that a few persons do survive being shot in the head, the large majority die; and it is my object to show you how a combination of physical and pathological experiments has revealed the reason why the majority do die, and revealed it, fortunately, so distinctly as to suggest means for warding off the fatal result.

(1) *Physical Considerations.*—First take the case of a bullet flying through the atmosphere. Here in this extremely beautiful photograph, kindly lent me by Prof. Boys, you observe that the bullet drives before it a wave of compressed air. Now this compressed air-wave is what is popularly called the wind of the shot, and to it used to be ascribed by military surgeons a certain proportion of deaths. The origin of this theory is difficult to discover, as the only case I am aware of in which the *post mortem* examination did not reveal hæmorrhage, fracture, &c. indicating that the shot had actually struck the body (though without injuring the highly elastic skin) is the instance given by the great Russian military surgeon Pirogoff, in his interesting surgical experiences of the Crimean war. Even this instance finds *à priori* a more reasonable explanation in syncope, and we shall see directly that the wind of the shot not only cannot, under any circumstances, kill a man, but also that its energy is far too slight for it to have any destructive effect whatever. It is rather curious to find that but few attempts have been made directly to estimate the wind of the shot, and those by Pelikan and others are only for large shot and by too coarse methods to be applicable in the case of a bullet, as the following experiment shows.

An extremely light vane of paper carrying a delicate mirror is suspended to a cocoon fibre, and carefully protected from currents of air in the room. A very gentle puff causes the vane to fly out most vigorously, yet we shall find that the ·380 bullet moving a thousand feet a second may pass within eight inches of it without causing the least deviation of a ray of light reflected from the mirror. It is only when the bullet passes within an inch or two of the edge of the vane that there is some slight rotation. The ·303 magazine service rifle, with a velocity of twice that of the larger bullet, produces little more than the same result. It is therefore obvious in this case that the far higher velocity is more than compensated for by the lesser sectional area of the projectile displacing the air. Although there was no proof of much displacement of the air, it was pretty generally held that when the bullet entered any substance the compressed air driven before it exercised an explosive effect. This opinion was more particularly supported by the Belgian physicist Melsens, who actually described it by the term "projectile air." The matter was taken up from the point of view of pure physics, and Magnus demonstrated that if a body like a bullet entered water, *e.g.* in falling the funnel which the displaced water makes in the axis of the body as soon as that is fully immersed, entangles air, and that it is this air which is carried by the body into the fluid, rather than that any air is forced in in front of the bullet. In answer to Magnus, Laroque invented the following ingenious experiment. He allowed a long body, incapable of wholly sinking, to drop into the water, and then found that there was air driven in in front of it; while, by the nature of the experiment, he had, of course, excluded the possibility of any air following the base of the projectile. I have repeated all these experiments (employing in Laroque's a slender rod of wood) and found that while his contention that air is driven in front of the bullet is completely substantiated, yet Magnus' observation is so far correct that air is also

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[1] A lecture delivered at the Royal Institution on April 6, by Prof. Victor Horsley, F.R.S.

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